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Solid Earth
Discussions

Interactive comment on "The deep Earth origin of the Iceland plume and its effects on regional surface uplift and subsidence" *by* N. Barnett-Moore et al.

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Reviewer: This paper concerns the prediction of the Iceland plume from numerical global mantle flow experiments constrained by plate motions in a mantle reference frame and next a comparison of predicted and published plume paths and of plume-driven dynamic topography evolution with observations of relative vertical motions of North Atlantic region. The manuscript is well written but nevertheless fails to be convincing on both topics (and I am sorry to conclude that). Basically, to allow for useful comparison between model predictions and observations insufficient material is presented to demonstrate the significance of the position and geometry evolution of the "model Iceland plume" as well as of the derived model predictions.

Authors: We will present more material on the position and geometry of the Iceland-like model plume in the revision, including several mantle flow models.

Reviewer: This needs proper attention because the mantle flow modeling is inherently approximate due to, e.g., implementing simple depth-dependent rheology, imposing a free-slip surface boundary condition, ignoring lateral buoyancy contrast in the upper few hundred km, and assuming an initially laterally homogeneous dense layer atop the CMB (to mention a few main approximations).

Authors: In the presented models, viscosity depends on temperature, pressure, composition and depth (see Eq. 1, p. 3, l. 24). Plate velocities are imposed at the surface in these mantle flow models. In the upper few hundred kilometers the time-dependent structure of the thermal lithosphere and shallow part (< 350 km depth) of subducting slabs, consistent with the tectonic reconstruction, are progressively assimilated in the mantle flow model (p. 4, l. 2-7; Bower et al., 2015). A free-slip boundary condition is used to compute dynamic topography, and lateral buoyancy contrasts above 350 km depth are only ignored when computing dynamic topography (p. 4, L. 9-16).

Reviewer: In mantle flow modeling these approximations are often made because mantle viscosity and initial mantle structure are largely unknown (e.g. see King 2016 for some contrasting inferences on mantle rheology) implying there is a large model space to explore. This model space is made smaller by the authors by constraining the surface with lithosphere plate motion (although the pertinent absolute plate motion model is not mentioned) and by imposing past subduction in a mantle reference frame. This however also brings new uncertainties in the problem because absolute plate motions are uncertain (see e.g. Doubrovine et al. 2012 for (large) uncertainties in absolute plate motion poles) and the implementation of past subduction (Bower et al. 2015) is approximate at best. Moreover, modeling of plume initiation is acknowledged in the paper as difficult due to what the authors call the “stochastic nature of plume dynamics” which requires the authors to tweak the model initiation times (or other) in order to have the Iceland plume hit the surface around approx. 60 Ma albeit still in the wrong location (approx. 10 degrees to the SE of Iceland). Then, in what kind of "plume" results the modeling effort in this paper?

Authors: The revision will include a revised section and additional figure, discussing the parameter spaced explored (initial model age and tectonic reconstruction) across several geodynamic models. This material will allow us to better expose and discuss the uncertainties inherent to the modeling approach.

Reviewer: Is the "model Iceland plume" actually a plume (in the sense of the 0-order geometry of a cylindrical upwelling with possibly a plume head)? I would not know. The model Iceland plume is not illustrated in Figures/Movies and no comparisons are made with existing suggestions of plume position & geometry from seismic tomography or with plume & mantle flow predicted from seismic tomography (e.g. Steinberger et al. 2014). In fact, based on the material presented I would suggest that the "model plume" is more akin to an upwelling sheet as I infer from inspecting the generally elongated SW-NE shape of predicted dynamic topography in fig. 3.

Authors: The revision will include an additional figure showing cross sections through the mantle flow models in order to better illustrate the morphology and evolution of this model plume, and showing that its geometry is plume-like as opposed to being akin to an upwelling sheet.

Reviewer: The Iceland plume/sheet is predicted in the wrong position and the model requires a uniform Euler rotation of 10 degrees to bring it to Iceland (and the LLSVPs and slabs and other plumes are then also rotated). Is the global model still Earth-like after rotation? Are there other plumes in the global model? Do they occur at/near known hot spots? Are remnants of subducted slab properly predicted and is the shape and position of the LLSVPs in accord with inference of seismic tomography? In summary: Is your global mantle flow model Earth-like in these aspects (such that Iceland plume evolution could be Earth-like)? These topics are all not sufficiently addressed nor demonstrated and the significance of geometrical evolution of "model Iceland plume" is elusive (for the reader).

Authors: We agree that the surface rotation of the entire global model is unsatisfactory, and that simulations are not 'Earth-like' as a result of this rotation. The revised manuscript will explore the influence of model parameters on the deep evolution of the mantle flow and the prediction of a model plume in the vicinity of the North Atlantic. We will select the preferred mantle flow model based on the predicted motion path of the Iceland-like plume. Instead of rotating the predicted global dynamic topography, we will: 1. Extract the dynamic topography for the first 60 Myr of evolution of the Iceland-like model plume. 2. Extract the motion path of the Iceland-like model plume over the last 60 Ma and rotate this motion path so that the model plume is under Iceland at present-day. 3. Remove the dynamic topography associated with the model-like Iceland plume between 60 Ma and the present-day. 4. Rotate the grids obtained in step 1 based on the motion path obtained in step 2. 5. Merge the dynamic topography fields obtained in steps 3 and 4.

This approach will preserve the dynamic topography associated with the fully dynamic Iceland-like model plume, and shift it in space and time to make the comparison with geological proxies possible. In the revision, we will compare the geometry of the Iceland plume and predicted dynamic topography for more than one model case.

Reviewer: Clearly a large set of key assumptions and uncertainties have determined the "model Iceland plume" as some approximation of the actual plume, but how good this approximation is remains unknown.

Author: Several mantle flow models will be presented to make key assumptions and uncertainties explicit.

Reviewer: This is the state-of-the-art of this type of mantle flow modelling and I would not be bothered so much by this if the plume position through time and the predicted evolution of dynamic topography would not play a central role in the story in comparing model predictions with observations and findings from others, which is another main theme of the paper. Either one fine-tunes a model to improve the fit with the observations or one estimates uncertainties in the model predictions to allow for a proper comparison. Neither is done here.

Authors: The series of global mantle flow models presented in the revision will allow us to estimate the uncertainties in the model predictions.

Reviewer: With respect to the vertical motion/displacement part of the paper I want to be brief: In Fig. 5-8, I am struck by the general degree of mismatch between the model prediction (blue) and the other curves and I can really not comprehend how the authors can see this differently. The zero-order approximation of the topography level generally mismatches, which the authors admit, while the first-order linear trends occasionally show similarities (but then not at the right topographic level!).

Authors: Dynamic topography predictions should be compared with anomalous tectonic subsidence rather than directly with topography (since the model does not consider variations in isostatic topography), and we will revise Figures 5-8 accordingly. In addition, we do not expect a perfect match between predictions and constraints across the whole North Atlantic domain. We will state the limitations of our modeling approach in the revision, and discuss the significant magnitude mismatch for several wells across different basins in the region. The model predicts long-wavelength vertical motions related to large-scale mantle flow below a given depth (either 250 or 350 km). Mismatches with anomalous tectonic subsidence in magnitude or timing could be related to other processes occurring in the upper mantle or lithosphere.

Reviewer: The authors do not always give a fair and balanced comparison with other work. For instance Iceland plume dynamics modeling based on back-advection of present-day density and temperature structure of the mantle derived from tomographic models (which suggests a different plume trajectory) is marginalized too easily by suggesting a potential problem with the fact that thermal diffusion is ignored during back-advection. This may be true for back-advection of small-scale structure (order 100 km), but back- advection of large and smooth mantle structures (> 500 km as derived from the tomographic models used) can be done in good approximation back to at least 60 Myr, which encompasses the Cenozoic Iceland plume evolution, because heat transport through conduction is so slow compared to heat advection. Of course, also that type of assessment of plume dynamics struggles with many key approximations (in fact quite similar concerning the mantle flow computations), but instead of negatively focusing on that work, the authors could have better shown some self-criticism with respect to the potentially huge uncertainty in 3-D geometry and position of their own model Iceland plume (path), which is not addressed at all.

Authors: The revised manuscript will contain a more balanced discussion of the benefits and limitations of our forward modeling approach and of back-advection models. We will compare the motion path of the Iceland plume predicted by a series of forward mantle flow models with that predicted by back-advection models.

Reviewer: In summary: Although I am in general a fan of this type of research, this paper does not sufficiently convince with regard to the significance of the results obtained.

Authors: We thank the reviewer for his/her constructive review, and trust that addressing his/her suggestions will significantly improve the manuscript.

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